AMERICAN JOURNAL OF PUBLIC HYGIENE
AND
JOURNAL OF THE
MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH
ORGANIZED 1890.

[The Association as a body is not responsible for statements or opinions of any of its members.]

Vol. XVI. (New Series. Vol. III., No. 1.) November, 1906. No. 4

THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH was organized in Boston, March, 1890, with the following objects: The advancement of sanitary science; the promotion of better organization and co-operation among local boards of health, and the uniform enforcement of sanitary laws and regulations.

THE JOURNAL OF THIS ASSOCIATION has for sixteen years faithfully reflected the views of the public hygienists of Massachusetts. With the November issue of the year 1904, the encouragement received from many hygienic quarters induced the Association to undertake the expansion of the Journal to a national standard, with the co-operation of many noted sanitarians as editors. A new title was selected, while the older and well-established title was retained also.

The dates of publication are FEBRUARY, MAY, AUGUST and NOVEMBER of each year.

THE JOURNAL will contain the papers read at the meetings of the Association, papers contributed by hygienists, report of the discussions, editorials, abstracts, reviews and hygienic notes of professional interest. Subscription rates, $1.00 per year. Reprints furnished at cost price.

All Communications concerning the JOURNAL, copy, proof, subscriptions, advertisements, etc., should be addressed to the Managing Editor, B. R. RICKARDS, 739 BOYLSTON ST., BOSTON, MASS.

All communications concerning the ASSOCIATION should be addressed to the Secretary of the Association, JAMES C. COFFEY, CITY HALL, WORCESTER, MASS.

All bills not relating to the Journal, and MEMBERSHIP DUES ($2.00 per year), should be sent to the Treasurer of the Association, DR. JAMES B. FIELD, 329 WESTFORD ST., LOWELL, MASS.

EDITORIAL.

With this issue the American Journal of Public Hygiene enters upon its fourth year. It is just three years ago that Dr. H. W. Hill undertook the Managing Editorship of the Journal of the Mass. Association of Boards of Health, expanded the Journal to a national standard, and secured the services of well known sanitarians to take charge of the several departments of professional hygiene. Under this broad policy the Journal has had a steady, healthy growth, and we now have as subscribers Boards of Health and hygienists in many of the states, in Canada and in Mexico.

In September of this year Dr. Hill resigned the Managing Editorship owing to the great distance intervening between his present location and the home office of the Journal. The committee on Publication of the Association accepted his resignation with regret.

The Committee, in reorganizing the Journal staff, have appointed Mr. Rickards Managing Editor, and have urged Dr. Hill to accept the position of Associate Editor.

The new Managing Editor will, therefore, have the services of one experienced in Journal affairs and the ever ready support of the Editor-in-Chief, Dr. S. H. Durgin, without which the present successful career of the Journal would have been impossible.

It will be the policy of the new Managing Editor to continue along the present lines, building up and enlarging the scope of the Journal as fast as its growth permits. The rate at which it grows will depend largely upon the support given by the public hygienists of the country.

OCTOBER QUARTERLY MEETING

OF THE

Massachusetts Association of Boards of Health.

The quarterly meeting of the Massachusetts Association of Boards of Health was held at the Brunswick Hotel, Boylston street, Boston, on Thursday, October 25, 1906. Luncheon was served at 1 P. M., and afterwards a business meeting was held under the presidency of Dr. H. P. Walcott.

On motion of Dr. Durgin the reading of the records of the July meeting was dispensed with.

Upon the recommendation of the Executive Committee the following gentlemen were elected members of the Association:

Fred A Bates, Lowell.
Thomas J. Beane, Lowell.
C. S. Millet, M. D., Brockton.
W. W Fullerton, M. D., Brockton.
Michael Duffley, Brookline.

The PRESIDENT: Is there any other business to come before the Association at this time? If not, the reading of the regular papers upon your programme will now be in order.

First is a paper upon "Gaseous Disinfectants," by Dr. H. W. Hill. The paper will be read by Mr. Coffey, in Dr. Hill's absence.

Mr. Coffey read Dr. Hill's paper, as follows:—

TERMINAL DISINFECTION—REASONS AND METHODS.

By H. W. Hill, M. D.
Minnesota State Board of Health Laboratory,

Disinfection is one of the weapons chiefly in use by the professional public hygienist at the present time in his warfare against infectious diseases. The principles on which it is based are logical; they have a strong grasp on the public mind; they provide reasons for doing something obvious, definite and

impressive. It is well worth while, therefore, to review these principles, to consider the methods by which may be accomplished disinfection in fact as well as in name; to list the diseases in which disinfection is usually practised, and finally to ask and answer if possible the question whether disinfection, logical and efficient, though it be, really restricts disease.

To begin with definitions. Disinfection is literally the destruction of the power to infect. In its widest sense, the term can be properly applied to any procedure whatever by which the infectiveness of anything possessing infectiveness is removed. Hence the boiling of instruments before surgical operations, the use of bactericides in infected wounds, the exposure of infected materials to sunlight, the efficient embalming of the infectious dead, the prophylactic treatment of infectious excreta, the steaming of infected clothing, even the pasteurization of infected milk, the filtration of infected water, the destruction of yellow fever or malaria-bearing mosquitoes can be properly called disinfection. In actual use, however, in public health circles the term has come to mean specifically the destruction of the infectiveness of the environment of infected persons. Hence in public health practice it relates to infected things and not to infected persons. Moreover, public health disinfection in practice is usually restricted still further to terminal disinfection, i. e., to the disinfection of the patient's room or house and its contents, after their vacation by the patient. The disinfection of excreta, clothing, eating utensils, etc., which is required daily during the course of the disease is usually left to the medical attendants, although these are as important, in many cases much more important, than terminal disinfection, and it is as much the duty of the public hygienist to devise by experiment and to recommend officially to physicians the best methods for these forms of disinfection as it is to do the same for terminal disinfection. I shall, however, confine myself to the question of room disinfection.

Disinfection disinfects by destroying the power to infect—

and this is usually accomplished by destroying these minute living organisms which are known to cause infectious diseases —or at least certain of the infectious diseases, and by inference and analogy, all of them. Disinfection differs, therefore, from sterilization, since sterilization means the destruction of all the minute living organisms, while disinfection means only the destruction of those minute living organisms capable of producing disease. Anything sterilized is, therefore, also disinfected, for if everything be killed, the harmful organisms die with the harmless ones. But things may be disinfected without being sterilized, if the harmful organisms are killed, while the harmless ones are left surviving. This distinction is worth making because methods of disinfection quite adequate for the destruction of harmful organisms, may at times fail to destroy more resistant but harmless organisms, the subsequent growth of which latter may mislead the observer, and induce him to believe that the disinfection was imperfect.

If then we agree that disinfection from the public hygienists point of view narrows itself down to the destruction of disease-producing organisms after their escape from the body, it is advisable to consider how they escape from the body, where they escape to, how long they live in their new location, what the chances are of their removal from those locations to well persons, while still capable of producing disease, and finally, if we can, to determine how often such transfer does actually occur. No amount of argument or experiment demonstrating the *possibility* of such transfer should distract the public hygienist from the sober and reasonable consideration of facts relating to the *probability* of its occurrence—still less from facts which tend to actually demonstrate whether or not the possibility or probability thus determined is actually realized in every day practice. Chapin of Providence has in recent years considered very forcibly this subject and his papers have done a great deal to pave the way for a dispassionate and logical consideration of the great question, "It is all right theoretically, but *does it work?*"

In principle, any disease believed to be due to a specific organism, where the latter may escape from the body, survive for a time in the place to which it escapes, and be transferred from that place to a well person, should be disinfected for by the application to all possible lurking places of efficient methods for its destruction. The following is a fairly complete list of the diseases in this country which should be disinfected for, if those principles are to be logically carried out—typhoid fever, diphtheria, grippe, dysentery, erysipelas, glanders, rabies, actinomycosis, tuberculosis, leprosy, syphilis, soft chancre, gonorrhea, gonorrheal ophthalmia, cerebro-spinal meningitis, tonsillitis, pneumonia, cholera, anthrax, plague, smallpox, measles, scarlet fever, whooping cough, tetanus, possibly acute rheumatism and cancer. Besides these specific diseases, there are a large variety of general infections, grouped roughly under the name of septicaemias, due to organisms capable of escape from the body and of survival after escape.

The points of exit from the body of the infective organisms differs somewhat in each case from a quantitative standpoint, but in general, the naso-pharynx and anus are the chief, the skin and the urine being involved, in some diseases always, in others at times. Those affections particularly affecting the internal mucous surfaces such as diphtheria, typhoid fever, etc., are chiefly infections through the corresponding excreta. It is a question whether or not measles and scarlet fever are infective from the skin, while little doubt of their infectiveness from the naso-pharynx can now be entertained. Leprosy is probably chiefly disseminated from the nose of the patient.

In face of this long list of infectious diseases, which might properly be disinfected for, it is interesting to note that American practice usually includes routine municipal disinfection for only four diseases—smallpox, diphtheria, scarlet fever, and of late years, tuberculosis. It is true that the long list given may be cut down by eliminating rare diseases, usually disinfected for when recognized; diseases not readily transmitted except through lesions of the skin, such as tetanus,

rabies, actinomycosis, and diseases known to be comparatively rarely transmitted otherwise than by direct contact, such as the venereal diseases. But even so, many diseases remain, for which terminal disinfection is not done, notwithstanding the general belief that they may be transmitted indirectly. The only reason why measles, whooping cough, grippe, dysentery, tonsillitis, etc., are not disinfected for is because of the usual belief in their mildness or because it is obvious that other methods of infection than through things far outweigh fomital infection. Work done and observations made, particularly by Chapin, tend to show that diphtheria and scarlet fever may soon be added to the list of those diseases whose transmission is so much more frequently direct than indirect, that disinfection to prevent the latter is scarcely a factor in reducing their prevalence.

Concentrating attention on the four diseases by common consent chiefly subjected to disinfection, we find that the infective agents in all four, smallpox, tuberculosis, diphtheria and scarlet fever, escape chiefly from the naso-pharynx. The skin in smallpox is undoubtedly infective; and it has been usually believed that the same is true of scarlet fever, although considerable doubt is now expressed on this subject. At all events, in all four diseases the infective agents escape chiefly, if not wholly, from the nose and mouth. They escape accompanied usually by the mouth and nose discharges. These discharges escape by voluntary spitting or blowing of the nose, by sneezing, coughing, laughing, and loud talking, etc., and by drooling. They are deposited chiefly in the immediate neighborhood of the patient, hence in handkerchiefs, the pillow and sheets, etc., the bed frame, floor near the bed, and to some extent especially the minute particles thrown out in sneezing, coughing, etc., almost anywhere in the room; also, since the hands go constantly, consciously or unconsciously, to the nose and mouth, things touched by the patients hands.

Being accompanied always by particles of saliva, if not by large masses of sputum or nose discharge, these organisms

remain wet for considerable periods wherever they happen to fall, the time during which moisture is retained depending on the humidity of the atmosphere, the nature of the surface on which they are deposited, etc., etc. While drying to the point of solidity usually occurs, drying to the point of pulverulency can seldom occur, and the bug-bear of"atmospheres of sickrooms laden with dust carrying the germs of disease" is not frequently realized. Indeed, when drying has been prolonged enough for particles of this nature to become broken up sufficiently finely to admit of being blown about and inhaled, the organisms contained in them will usually be dead. On the other hand, attrition by passing feet, brushing, etc., may dislodge them when still moist, but if so, they will be in relatively large and heavy fragments, falling quickly to the floor and unlikely to reach the one great portal of entry to a new victim—the naso-pharynx. Hence the chief danger to be feared from such patients, apart from direct transfer of sputum, etc., as the hands to the mouth, is the transfer of the minute particles coughed or sneezed out, which float for a time in the air, and may be inhaled while both fresh and moist. Sputum is not abundant as a rule, except in the case of tuberculosis and the results of drooling at the mouth are likely to be confined to the immediate clothing, etc., of the patient. Moreover even coughing, sneezing, etc., are much more common in tuberculosis than in diphtheria, scarlet fever or smallpox. Hence it is, that from purely *a priori* reasoning it would be proper to infer that, apart from the bed, bedding, clothing, handkerchiefs, etc., of the patient, the infective agents of these diseases other than tuberculosis will not be scattered widely or abundantly—and actual experiments by myself confirm these results at least for tuberculosis and diphtheria. Again it would be proper to infer that the chief scattering would occur in tuberculosis, and this again has been found true comparing the same two diseases. Finally it would be reasonable to suppose that the organisms thus scattered would die rather quickly from simple drying; and

in diffuse daylight (the conditions usually obtaining in the rooms of most patients) neither tubercle bacilli nor diphtheria bacilli could be counted on to survive three weeks, in my own experiments. In general then, when the disinfector enters a room just vacated by a patient suffering from diphtheria or tuberculosis (and by inference the same is true of smallpox and scarlet fever), the air of the room may contain a few organisms floating about, having just escaped from the naso-pharynx of the patient in the last few coughs, sneezes, etc., of the patient before leaving. Smears of sputum in all stages of dryness may be encountered upon the pillows, etc., and dry smears upon the furniture, etc., near the bed, placed there by the patient's hands. But the greater part of the infection of the room is concentrated about the bed, and in the rest of the room it is quantitatively small and qualitatively weak, particularly in diphtheria. In tuberculosis, for reasons given below, this factor is more serious than in diphtheria. Terminal disinfection is usually done after recovery in diphtheria, scarlet fever and smallpox, since recovery is the majority rule in these diseases. The great majority of the bacteria, etc., thrown out by the patient certainly in diphtheria, probably in scarlet fever and smallpox are thrown out chiefly in the acute stages of the disease, which is long past before terminal disinfection is done. Most of these organisms have died a natural death by the time the case is released from quarantine. Of course the patient has doubtless continued to throw off organisms during convalescence but fewer and fewer as the days go by. In the case of tuberculosis however terminal disinfection is done much more frequently after death or removal, and thus during the height of the disease than in smallpox, scarlet fever or diphtheria. Hence in cases of tuberculosis, the infection is usually quantitatively greater up to the time of disinfection than in the other cases, absolutely and relatively.

On these grounds it seems to me that whatever reasons may be offered for the disinfection of smallpox, scarlet fever

and diphtheria, apply much more forcibly to tuberculosis, and that those who regard as strong the arguments against terminal disinfection after the first three diseases must admit that tuberculosis offers *in esse* a very different proposition. Personally I believe that disinfection after tuberculosis should always be done, if the room of the patient is to be occupied by a well person within three weeks, and I would prefer to have the limit three months.

Admitting, then, that terminal disinfection of rooms is a proper weapon of the public hygienist at least against tubercular infection, it remains to discuss the methods to be adopted. Disinfection may be accomplished by applying to the infected surfaces any agent which will kill the bacteria. While sunlight, steam or dry heat of sufficient intensity, will kill bacteria, these can seldom be successfully applied to all parts of the interior of a room. Washing with disinfectant solutions is a laborious task, and so long as we assume that any surface or crack or irregularity of the floor, wall, or furniture may carry the infective agent, washing cannot well be trusted, for the washer may wash everything except the fatal point. Disinfectant gases have the advantage that they do not weary in well-doing or object to getting in behind heavy furniture. They have no regular hours of work and do not overlook cracks or crevices anywhere. Primarily gases are used because of the universality and uniformity of their action and because they apply themselves to all parts of the room and to all irregularities of mouldings, etc.

Many gases are highly efficient when properly used. Sulphur dioxide long held the first place, but chloride, hydrocyanic acid, even carbolic acid vapor are efficient. All act, however, immensely more effectively in humid than in dry atmospheres. Indeed, in absolutely dry atmospheres they are inert. Sulphur fumes and chloride are objectionable on account of their action on metals and fabrics, hydrocyanic acid is too deadly a poison for routine use and carbolic acid vapor is probably too expensive, when used in sufficient concentra-

tion. Formaldehyde has now been almost universally adopted on grounds of cheapness, innocuousness and efficiency, so that practically consideration of the other gases may be dispensed with.

Formaldehyde is a gas of peculiarly pungent and striking odor. Commercially it is to be had as a (nominally) 40 per cent solution by weight in water. It is worth noting that it was in this form that it was first introduced as a disinfectant and to the fact that it was at first thus invariably associated with water is due I think its initial success as a disinfectant. Had formaldehyde been introduced otherwise than as a watery solution, the evaporation of which was essential to the liberation of the gas, humidity would not have been secured, and formaldehyde would have been ranked as no more efficient than sulphur or chlorine. After its reputation was established by experiment with the liquid form, solid forms were introduced, (the result of concentration of the solution, so that polymerization occurs). From these polymers, the gas may be liberated by heat, but since almost no water is evaporated with the gas, the polymers are relatively inefficient, unless water be added from an outside source. Since for equal quantities of available gas, the polymers are usually very much more costly than the liquid solutions, I see no advantage in their use for ordinary municipal purposes and will not describe the numerous mechanically ingenious devices for their use.

Successful room disinfection at the present time, then, requires the presence in the room of sufficient gas to fill the unoccupied spaces and in sufficient concentration to kill the organisms, together with the maintenance of these conditions for sufficient time to ensure the complete action of the gas. The liberation of the gas in the room has been accomplished by a large variety of devices. These may be classified as mechanical, thermal, chemical and thermo-chemical methods. The mechanical methods may be illustrated by the spraying of formaldehyde by air pressure; in Chicago, a small bottle and

atomizing device, capable of discharging a very fine spray upon sheets, hung on lines across the room proved satisfactory to the local authorities for a long time. In Philadelphia a much larger device for direct spraying into the room through the partly opened door is in use; while I myself devised a large atomizing apparatus, in which the atomizing agent was steam instead of air, the steam furnishing the required high humidity as well as the projecting force. Thermal methods range from the simplest form of pan, from which the formaldehyde is boiled off by a direct flame, to complicated devices intended to bring the formalin to the boiling point in small successive quantities, thus avoiding the polymerizing which results when large quantities are treated for considerable periods. Autoclaves in which the formalin solution was heated in a tight receptacle until pressure of one or more atmospheres was secured had a great vogue at one time. In general however most of these devices have failed of general adoption because of high initial cost, awkward size or shapes, complication of design or inefficiency.

The chief chemical methods are two in number. One, the unslaked lime method, depends in principle on the fact that since formalin is a solution of the gas in water, removal of the water will set the gas free. Unslaked lime was used to take up the water, but it was soon found that the chemical reaction involved the destrution of the formaldehyde. Later the addition of alum was found to prevent the destruction of the formaldehyde, and the method then proved an excellent one for the liberation of the gas. Unfortunately, the very fact that the gas is liberated by the chemical abstraction of the water involves the non-liberation of the water in the room—hence the humidity so valuable in disinfection by any gas was not secured. Very favorable reports accompanied the early recommendations of this method but my tests failed to secure similar results. On investigation I found that the favorable experiments were based on the use of test bacteria, dried so long before use that they refused to grow, even without ex-

posure to the gas. The second chemical method consists in mixing permanganate of potash with formalin. The result is a violent chemical reaction, between the permanganate and part of the formaldehyde, such that heat is evolved so great as to evaporate the remaining formaldehyde and water more or less completely. A good deal of work has been done in the attempt to determine the relative proportions in which these two chemicals should be mixed in order to secure the maximum liberation of gas, with the minimum destruction of gas. If too much permanganate is used, a correspondingly large amount of formaldehyde is destroyed, leaving too little to be evaporated. If too small an amount of permanganate is used, the amount of heat generated is not sufficient to evaporate the whole of the remaining formalin. I have appended a note which indicates the various proportions recommended by various investigators. The problem is not quite so simple as might appear at first glance, because the relative proportions of the two ingredients required for maximum efficiency at minimum cost varies according to the method of mixing the reagents, the physical condition of the permanganate and the initial temperature of the container in which the reaction occurs. As to the cost of this method as compared with other methods of setting free formaldehyde, estimates vary, and must be worked out in each community for itself. A community which already possesses a regular disinfecting staff and equipment will not greatly benefit by adopting this method because the initial cost of other apparatus, etc., has already been paid, and because the proposition to reduce the staff on the plea of the less time and labor involved on the new method would not usually be entertained. Moreover since much formaldehyde is used up and some wasted in the chemical reaction, it is in general necessary to provide about twice the amount of formaldehyde for the permanganate method that would be needed if the formaldehyde were driven off by other means, such as boiling over a flame. It will easily be seen, then, that the health department which wishes the principle

of maximum possible efficiency at minimum necessary cost to rule in its business affairs must do a great deal of careful figuring on prices of labor and time expended as well as apparatus and materials used before deciding this question. I may say that in my own opinion the whole problem of conducting municipal disinfection no longer hinges on whether the infective organisms can or cannot be killed by a given method. The infective agents can be killed by any one of fifty or more methods, there is no longer any dispute on that point. The problem now is not merely to achieve the killing, but to secure the killing at the minimum of cost, and the items to be considered are not by any means alone those of apparatus or materials, but also time, labor and accessories such as horses, wagons, drivers, disinfectors, helpers, etc., etc. These necessarily vary in each community, according to local rates of wages, etc., and according to the amount of work to be performed.

I suppose that the question which most prominently is present in your minds at this moment is—how much formalin should be used per 1000 cubic feet? This is the most difficult of questions at the present. It would seem to be a simple matter to determine the facts experimentally. Chicago long claimed the best of results from five ounces per thousand feet, while Pittsburg admitted occasional failures from 73 ounces per 1000 feet. The experiments of the state of Maine on the permanganate method showed an extremely high rate of efficiency with 32 ounces per 1000 cubic feet, while in my own experiments, confined, it is true, to the diphtheria and tubercle bacilli, 1 ounce per 1000 cubic feet showed perfect results. The unslaked lime method already described was recommended as highly efficient at 6 ounces ped 1000 cubic feet, while I failed to kill at all uniformly with 10 ounces per 1000 cubic feet. The fact is that no fixed amount can be stated without qualification, except an amount of solution so large that its evaporation involves the evaporation also of sufficient water to raise the humidity to a

high point. I think that this was the secret of the success of the Maine experiments—and that the use of the permanganate to which their success has been inferentially attributed was a mere incident. I am confident that equally good results could have been secured by a similar evaporation by other means of a much smaller quantity of formalin, combined with sufficient water to make the same total of 32 ounces.

The basic factors of successful disinfection and by that I mean maximum efficiency with minimum expenditure are: First, sufficient gas; second, action of this gas for a sufficient time. The accessory factors all relate to securing these two. The accessory factors are rapidity of evolution, (in correlation with the leakiness of the room and the amount of absorbing surface exposed), the amount of humidity present and finally the kind and condition as regards dryness of the organisms which are to be killed. The condition of the exposed organisms as regards dryness is most important. A fresh organism (i. e. taken from a recent active culture) if moist, is readily killed even by a dry gas (i. e. in the absence of atmospheric humidity) because the moisture necessary to the action of the gas is supplied by the moisture of the organism. A fresh organism, rapidly dried, is insusceptible to the action of a dry gas, but readily succumbs to a moist gas (i. e. gas liberated in an atmosphere of high humidity). But an organism dried for a considerable time (varying with the organism, say a week or ten days for the diphtheria bacillus) is so weakened by the drying, that even if not killed by that alone, it succumbs readily to the action of relatively small quantities of gas at relatively low humidities.

In my own experiments, I have been able to adjust the factors of humidity of and of dryness of organism so that in any given experiment, I could secure any result which I pleased, irrespective, within limits, of the amount of gas used, so that if beforehand ten ounces of formalin were prescribed, I could arrange matters so that all the organisms were killed,

half of them killed or all left surviving, just as I pleased, using the same cultures in all cases. Similarly if one ounce per 1000 feet be prescribed, I would undertake to kill or to leave surviving at will the organisms exposed, provided that I am allowed to control the other factors of leakage, rapidity of evolution, humidity and condition as to dryness of the exposed organisms. Even in an air tight room, thus cutting out the leak factor, I would undertake to secure similar results, under the same conditions. I know this is so because I have repeatedly done just this.

Now it is little wonder that in so-called "practical experiments" where four of the six important factors, leakage (and absorption), humidity, rapidity of evolution and dryness of organism do not receive proper attention, (i. e., "experiments" in actual rooms occupied by patients) investigators have reported the most contradictory and perplexing results. It would be amusing if it were not alternately exasperating and pathetic to see results quoted from a given experiment as due to the use of a certain machine, a certain method, or a certain preparation of formaldehyde, instead of to the particular combination of the above stated factors which happened to obtain at the time. A given quantity of formaldehyde gas in a given space does not depend for its activity on how it reached that space—or whether it came from this or that machine or by this or that method. The real accessory factors of the results achieved are the length of time it is allowed to act in this concentration, the humidity, and the work which it is called upon to do.

It is necessary then to determine the average conditions concerning these factors which obtain in practice. In actual practice, the condition regarding dryness of the infective agents may be argued out as follows—at least for diphtheria and tuberculosis. Those organisms thrown out by the patient up to three weeks previous to the disinfection may be counted on as mostly dead. Those thrown out within two or three days preceding disinfection may be counted on

as fresh and active, and more or less moist. Those thrown out between these two periods are probably alive, and dry, but their vitality has been more or less impaired, increasingly so with the amount of time elapsed. Now the fresh moist organisms thrown out recently are very readily killed by formaldehyde gas; those over a week or ten days old are so decrepit merely from drying that they readily succumb. Hence only those thrown out for say the week preceding disinfection need give any difficulty. These are dry and therefore so far resistant, while they have not yet been weakened by the dryness. Hence it is on these organisms that the attention of the investigator should be concentrated. Whatever will kill them will kill the others.

Hence it is that the humidity factor must be looked out for in practical disinfection—because the organisms which are most to be feared are in this condition of dryness, not indeed to the point of pulverulence, but to the point of resistance to gas. Concentration of the gas is to be provided for first by stopping all leaks in the room, and second by securing rapid evolution. It is easily seen that these are correlative. Slow evolution does no harm if there be no leaks (or absorption), because the proper concentration will be achieved in time. But with slow evolution and a leakage (or absorption) equivalent in rapidity to the rule of evolution, the desired concentration could never be obtained. Now leakage, and absorption, can never be wholly excluded in practice and therefore rapid evolution is always essential. In this connection I am glad to refer to the ingenious method of Dr. Wallace Clark, H. O., Utica, N. Y., who allows formalin to flow from a douche bag upon the hot fire pot of the ordinary hot air furnace, the gas being transmitted to the rooms through the heating pipes. Although of obviously limited applicability it permits, when available, the rapid evolution and high humidity essential to success in practice.

As to the final factor of length of time of action, this again depends on the amount used, and the humidity secured. In

general I believe that twenty ounces of formalin per one thousand feet, rapidly evolved, can be counted on to produce complete disinfection in the ordinary diseases disinfected for on one thousand cubic feet of space, within two hours; that ten ounces of formalin with ten ounces of water will accomplish exactly the same results and that five ounces of formalin with fifteen ounces of water will do just as well, always provided that leakage is well controlled. In thus recommending five ounces of formalin and fifteen ounces of water, I am allowing as a margin of safety five times the amount of formalin that I have found efficient in laboratory experiments. In the rare cases where anthrax spores or other exceptionally resistant organisms might require disinfection, it would be no harm to double these quantities, and the time of exposure, although I cannot speak with experimental accuracy on this point.

As to methods of evolution, so long as they are safe and rapid they will succeed, and economy is the only remaining point to consider. Where the wetting of the room and its contents with a finely atomized spray is not objectionable, a spraying device offers the quickest and best means. The steam atomizer which I devised had advantages of this kind, offset however by the size, weight and, for unskilled labor, relative danger of the apparatus, which carried a head of fifty pounds of steam. Any device resembling an exaggerated kettle, the spout of which can be applied through the keyhole, or the hole left after removing the handle of the door, or even through the partially opened door, the opening being appropriately draped with a wet sheet to avoid escape of gas will secure excellent results, always provided that the flame used is sufficiently powerful to rapidly evaporate to dryness the whole contents of the kettle. Open receptacles to be heated by a flame and to be left in the room are objectionable only because of the danger of fire and the difficulty of so adjusting the lamp that it will evaporate the contents of the receptacle completely, and yet not injure the receptacle or the

room by continuing to heat it after the evaporation is complete.

The chemical methods outlined above are excellent, so far as evolving the gases are concerned, but the unslaked lime method reduces the humidity otherwise available from the formalin itself and the permanganate method requires an excessive amount of formalin to allow for that destroyed in the reaction. In both methods economy of formalin demands accessory apparatus for securing humidity.

If I may add a paragraph or two regarding the disinfection of public conveyances, sleeping cars, street cars, etc., I would like to say that it seems to me that the disinfection of these rests on a somewhat different basis from the disinfection of ordinary rooms vacated by infectious patients. The public conveyance is offered to the public as a conveyance, and not as a room recently occupied by infective persons. If occupied by patients who are infective, they are likely to be reoccupied immediately afterwards, when the organisms thrown out by the patient are still fresh and moist; they are occupied by all sorts and conditions of persons, unsuspicious of the recent infection—and finally and chiefly the seats, etc., which the infected patient occupies are more nearly analogous, so far as the likelihood of infection is concerned, to the pillow and bedding of the ordinary patient's room, than to the furniture, chairs and tables of the latter, to which they are more likely to be compared. There is no question in the minds of any opponent of municipal house disinfection that pillows and bedding should receive disinfection before subsequent use, and I think that the occupation of an undisinfected plush-lined seat in a railroad car immediately after its vacation by an infected patient is somewhat like getting into a diphtheria patient's bed, without disinfecting the sheets. As to the disinfection of sleeping berths and contents after such occupancy, there can be no question.

In summarizing this paper I would like to emphasize the following points:

1. The basic factors of successful disinfection are sufficient gas, acting for a sufficient time.

2. To secure these, the stoppage of all leaks and rapid evolution of gas will alone secure the proper concentration of gas; while humidity is essential to economy of gas, and to economy of time.

3. What constitutes sufficient gas, sufficient time of exposure and sufficient humidity, cannot be stated as fixed for any one factor unless the other factors be taken into account also. They are all correlatives of each other and depend finally on what results it is desired to achieve.

4. With the ordinary pathogenic organisms ordinarily disinfected for and rapid evolution, twenty ounces of formaldehyde will meet all ordinary conditions, while ten ounces of formaldehyde with ten ounces of water or five ounces of formaldehyde with fifteen ounces of water will act just as well, because all three prescriptions supply enough gas and enough humidity.

5. For the permanganate method, twenty ounces of formalin should be used in order to secure both gas and humidity in sufficient quantities. I am not aware that a method for using permanganate in which less formaldehyde and more water can be evaporated by the chemical reaction alone has been devised. If this is done, however, the amount of formaldehyde may be reduced, the total of twenty ounces being maintained.

6. Gas evolved from solid forms of formaldehyde or otherwise obtained in a dry form is just as good as, but more costly, usually, than gas evolved from moist sources, if the necessary humidity be provided also by an accessory operation.

7. We already admit practically by inaction the futility of disinfecting for the great majority of the acute infectious diseases. Smallpox, scarlet fever and diphtheria are probably to be included with these. But, for reasons given, chronic infections with a high mortality, such as pulmonary

tuberculosis, leprosy, etc., or where particularly active organisms with unusually high resistance, like anthrax, are concerned, should continue to be disinfected for.

8. The disinfection of public conveyances especially those having seats, hangings, etc., paralleling the conditions of a patient's bed, should be disinfected after each known use by an infected occupant.

References:

Evans and Russell (Maine State Board of Health, 1904) recommend 32 ounces of formaldehyde and 10 ounces of potassium permanganate, claiming that 80 per cent of the formaldehyde present was liberated.

Hill (Boston Board of Health Laboratory) working on the same problem two years earlier, concluded that the results were extremely irregular, but that a much smaller amount of formaldehyde than 80 per cent. was liberated, the exact amount varying very much.

Salmon (American Medical Journal, April 15, 1905) repeated the work of Evans and Russell, finding that 26 per cent. only of formaldehyde was liberated, 33 per cent. destroyed in the reaction and 41 per cent. left unevaporated. Salmon advised mixing in the proportion of 6 parts of formalin to 5 parts of permanganate, finding that in this proportion 51 per cent. was given off, 39 per cent. destroyed, 10 per cent. left unevaporated.

Frankforter & West (Journal American Chemical Society, Sept. 1906) find that the fineness of division of the permanganate and the rate and manner of addition of the formalin greatly affect the result. They advise mixing the permanganate with sand (equal parts) and using this mixture in the portion of 2 to 1 part of formalin. Allowing 30 minutes for the addition of the formalin, they secured from 70 to 80 per cent. of formaldehyde liberated, but if such a slow method of adding formalin were adopted, the rapid evotion and simplicity of apparatus would be done away with.

Base (Journal Am. Chem. Soc., Aug. 1906) gives a number of experiments with permanganate and formalin, but it would seem from Frankforter's experiments that Base limited himself to mixtures in proportions not the best available for the purpose in view.

The Illinois State Board of Health issued a circular recently (1906) on "Practical Disinfection," in which 16 ounces of formalin and 6 3-4 ounces of permanganate are recommended (in a 10 or 12 quart pail) for use at temperatures above 60° F. It seems obvious that the whole subject needs careful re-examination with attention to all the varying factors outlined above before "Maximum result from minimum expenditure" can be attained.

Additional References.

Chapin—Annual Reports of Providence Board of Health, 1900-1905. Journal Am. Med. Assn., Aug. 25, 1906.

Hill—Trans. American Public Health Assn. 1902, p. 209.

Hill & Rickards, Trans. Am. Public Health Assn. 1902, p. 509.

THE PRESIDENT: The papers of this afternoon are so related in their scope that it has been decided that we

had better postpone the discussion till after the reading of Dr. Chapin's paper. We will, therefore, now call for the reading by Director Rickards of the Boston Board of Health Laboratory of his paper upon the "Value of Two"—not three, as written in your programme—of "Two Consecutive Negative Cultures in Releasing Diphtheria Patients." Mr. Rickards:

THE VALUE OF TWO CONSECUTIVE NEGATIVE CULTURES AS A METHOD OF RELEASING DIPHTHERIA PATIENTS.

By B. R. Rickards, S. B.,
Director Boston Board of Health Laboratory.

Mr. President and Members of the Association:—

You have listened many times to papers on Diphtheria and the problems which this disease presents to those connected with public health work. In fact, this Association has listened on two separate occasions to papers with somewhat similar titles. The first by Dr. Ernst was read in 1897, about a year before the regulation requiring two consecutive negatives went into effect,—the second by Dr. Hill, was given after the regulation had been in force a number of months.

In the eight years which have passed since that time we have had a chance to study the practical workings of the regulation in detail. It is my purpose to present to you some of the original reasons for the adoption of this regulation, to give some statistics which are now available bearing upon it and to compare it to some extent with other methods of release.

Three methods are in common use to-day for releasing from isolation patients convalescing from diphtheria. The first of these may be called the time limit method, where patients clinically well are released a certain number of days after the membrane disappears from the throat. This method was, before the advent of modern bacteriological

methods, in more common use than it is to-day. After diagnostic bacteriological laboratories began to be established a single negative culture was usually demanded before setting the patient at liberty and this method is to-day probably more common than either of the other two. The third method, that of requiring two consecutive negative cultures, was the outgrowth of the single negative method.

The time limit method being a purely arbitrary one has the disadvantage of releasing some patients while they are still capable of carrying infection and of holding others after nose and throat are clear of the diphtheria bacillus.

Were bacteriological methods perfect a single negative culture would be a sufficient safeguard in releasing diphtheria patients from isolation. Unfortunately, the working error in taking release cultures is comparatively large. While a positive culture is under ordinary circumstances convincing evidence that the patient is infected with *Bacillus diphtheriae*, a negative result has no such conclusive meaning. In laryngeal cases for instance there is difficulty in reaching the infected area. Moreover, the swabs once infected may not yield positive results owing to some flaw in the technique or, more likely, to overgrowths by some rapidly developing organism. Dr. McCollom of the South Department of the Boston City Hospital has convincingly shown that even in well marked cases a negative may be obtained unless care is used in taking the culture. If this be true of cultures for diagnosis how much more is it true that a single negative culture for release may not show the true facts of the case. Here there are no pathological conditions to guide one to the infected places, in fact in some cases the throat may be temporarily free from the diphtheria organism, the latter having retreated into the crypts of the tonsils. Again, if the case appears clinically well there is hardly the same incentive to thoroughness as there was, say, at the time of taking the diagnosis culture.

If then, the bacteriological error on a single negative is so

large, why change from the arbitrary standard, or why not accept the certification of the physician that the patient is well? Simply because the bacteriological error is less than the clinical error, since in perhaps a majority of cases the patient is clinically well long before the organisms have disappeared from the throat.

In order to show the facts regarding the single negative for release I desire to present the following figures:—In 1898 out of 1002 persons released by the laboratory 201 or 31 per cent secured a negative culture which was afterwards followed by a positive. In the first six months of 1900 this number was 154 out of 455 or 34 per cent,—while during the first eight months of the fiscal year, 1906 (February to October) 76 out of 497 or 15 per cent would have obtained a premature negative. In other words it may be safely said that of those persons who are released by the laboratory that from 15 to 34 per cent would have been released while still capable of infecting others if release were given on a single negative. I use the phrase "while still capable of infecting others" advisedly. Virulence tests made on diphtheria bacilli isolated from patients' throats shortly before the throats finally cleared, have in the majority of cases proved positive. This fact has been proved by many workers. Figures on this point also show a remarkable agreement. Wilson in Brooklyn previous to 1898, testing every case for virulence that ran over four weeks found 77 per cent of organisms isolated virulent. The Mass. State Board of Health a little later found 75 per cent of cases tested, virulent, and of the cases tested in the Boston Board of Health Laboratory during the last eight months 9 out of 12 cases, or 75 per cent, were virulent. Of the remaining three negative cases one was on an organism isolated from the ear, the second from the nose and the third was from a bacteriological case. With the exception of a few cases of long duration and in so-called bacteriological cases where the clinical signs of the disease are few or missing, the virulence of the

diphtheria bacilli seems to persist up to the time of their disappearance.

Unfortunately definite figures are not available to show what percentage of cases in private practice are released prematurely on two consecutive negatives. Undoubtedly the number is very small, probably considerably less than three per cent. A number of years ago three consecutive negative cultures were required by Dr. McCollom before a patient could be released from a diphtheria ward of the Boston City Hospital. So extremely rare, however, did he find a positive result following two consecutive negatives that after a fair trial the third negative was dropped.

In Boston it is required that for release a culture must be taken from both nose and throat and that after the first negative is obtained by the attending physician the next culture shall be taken by a medical agent of the Board of Health, the two cultures not to be taken on the same day. Each of these points is of undoubted value. We often find the nose infected with virulent bacilli for a considerable time after the throat is free from the organisms. Organisms from the nose may of course reinfect the throat or infect others. The throat and nose may both be apparently free one day and yet be found to be infected the next. But by far the most important clause in the requirement however is that referring to the medical agent. The cause of this is not far to seek. The family is naturally anxious to have the patient released from isolation, the attending physician is also naturally anxious to have the case clear up rapidly after the clinical signs have disappeared, in fact often finds it exceedingly difficult to explain to the family how after the disease of diphtheria has disappeared, the patient can possibly be a source of danger to others. Both patient and physician may consider the requirement a piece of red tape, and it is perhaps not strange if, after a long, tiresome case, his culture taking is not as thorough as it might be. The medical agent with his finer technique, perfected by constant practice and with no per-

sonal interest in the case sieves out those positive cases which have passed the screen of the attending physician. Such cases are then referred back to the latter until a negative is once more obtained, after which the medical agent again makes his test.

During the first six months of 1900, a year when diphtheria was very prevalent in Boston and elsewhere, of those cases released by the laboratory the average duration from the earliest symptoms as noted by the physician to the second negative culture for release was 23.3 days. During 1905, a year when diphtheria was comparatively light the average duration from the first positive culture for diagnosis to the second negative for release was 14.2 days. Allowing 2.6 days, the average time as found in 1900, as the average time elapsing between the earliest symptoms and the first positive we have an average duration of 15.8 days in 1905 as compared with 23.3 days in 1900, an average shortening of period of isolation by 6.5 days or practically one week. Considering these figures it seems fair to assume that if ten days to two weeks after the disappearance of the membrane be taken as the arbitrary standard for releasing patients, one would in the majority of cases be holding the patient longer than was necessary, since the average length of a case, in this city at least, is now but two weeks..

In connection with the shortening of the average duration of cases, one's mind naturally reverts to the fact that in the last few years antitoxin has been furnished free to all cases wishing it with the triple result that its use has greatly increased, that large doses are more often employed and that it has been more frequently given at an early stage of the disease.

In conclusion, it seems to the speaker, that the second negative method, while not perfect, is to-day the most satisfactory and scientific method of releasing diphtheria patients particularly when the second negative is taken by a medical agent of the Board, and where a virulence test is available for cases of long duration.

THE PRESIDENT: The last paper of the afternoon is Dr. Chapin's paper upon "Methods Employed in Providence for Releasing Diphtheria Patients." I have the pleasure of introducing Dr. Chapin.

THE METHOD OF RELEASE OF DIPHTHERIA PATIENTS IN PROVIDENCE.

By Charles V. Chapin, M. D.,
Supt. of Health, Providence, R. I.

Sanitary practice ought to be based on what we know of the origin of disease. According to the best evidence the contagious diseases extend not from known cases, but from those that are unrecognized, and from well persons infected with the disease, whom the English call "carrier cases." Infection usually does not take place except by quite direct contact. Fomites plays but little part in it. Unless these premises are accepted you will take little interest in the argument. It is only recently that the views expressed above have received any recognition. Until within a very few years we in Providence, as well as everybody else, believed that if we could only isolate every recognized case of contagious disease, until the patient appeared to be well, and disinfected the premises, we could stamp out the disease. In accordance with these views, in company with other Health Officials, I strove for years to increase the stringency of restrictive measures, but it became gradually clearer that even absolute control of reported cases of disease could accomplish only a moderate reduction in its prevalence. If this was so it seemed unreasonable to insist on such severity, and it appeared not unlikely that equally good results could be obtained by less drastic measures. The medical profession, and the public also, were seriously questioning the value of the measures usually adopted to control the contagious diseases, and were vigorously opposing the long continued isolation of well persons, even when it was demonstrated that they were the carriers of infection. It was considered unjust that the few well persons infected with diph-

theria bacilli, and coming to the knowledge of the Health Officer should be isolated, while a much larger number of infected persons were certainly known to be mingling freely with the public. So firmly had I become convinced that our regulations were unnecessarily severe, and that as much practical good could be accomplished by dealing more leniently with reported cases, that in 1902 it was determined to modify our methods to bring them more in accord with our latest knowledge of the epidemiology of these diseases.

Previous to 1895, and before the use of cultures, it was customary to terminate isolation in diphtheria one week after the attending physician certified that the patient was well. In 1895 the rule was adopted of requiring one negative culture from the throat of the patient before the release. In 1896 one negative was required from the throat of every member of the family. This procedure met with great opposition; it also demonstrated that a very considerable proportion of the well members of the family were infected. We never required two negatives in Providence. Early in March in 1902 our present system was adopted. Since then we have taken down the warning sign ten days after the disappearance of the exudation, as certified by the attending physician, on a postal card sent to him for that purpose. But no children in the family may attend school until one month from the beginning of the case, unless two successive negative cultures from both throat and nose are obtained.

Almost all the children come to the health department for the purpose of having these cultures taken, and, of course, many of them are found infected. Of course the period of isolation has been reduced by the adoption of these rules. Formerly 57 per cent of our cases were isolated over three weeks, but now only 27 per cent of the cases are isolated beyond this period. The average period of isolation under these rules is 20.13 days.

Our plan is doubtless illogical. Infected persons are isolated for a while, and then allowed to mingle with the public,

and even go to school, but that after all is what every health official permits. No one can possibly isolate even the sick until it is absolutely certain that they are no longer harboring diphtheria bacilli and no one even attempts to do this for infected well persons either in the family of the reported case, or elsewhere. Isolation is never absolute. The degree to which it is carried varies with the views of health officers and prosecuting attorneys. Even where the most stringent measures are carried out it is certain that many sources of infection remain uncontrolled, otherwise the disease would be eradicated. If absolute control is impossible, it is simply a question of judgment where the line shall be drawn. To make deductions from the comparison of statistics is difficult. It is impossible to show by such a comparative study that the amount of contagious disease in any community bears any relation to the stringency with which restrictive measures are carried out. The problem may be briefly stated thus: Since it is impossible to control absolutely every source of infection, how far is it wise to attempt to carry out our restrictions? I believe that both theory and experience justify the adoption of more lenient measures than are usually advised, therefore, we did make such changes in Providence.

It is only in the routine management of contagious diseases in cities that these moderate measures are advised. Incipient outbreaks in small communities, in public institutions, and even in day schools, may often be successfully controlled by an active interference which is not desirable under ordinary conditions. Thus where a child with diphtheria has been removed to the hospital, and treated there at considerable expense, largely for the sake of protecting the children in the family remaining at home, it is advisable to be quite sure that the patient is free from infection before discharge; therefore two, and even three negative cultures from throat and nose are recommended in such cases, and two cultures are required in Providence .

It will doubtless be asked, what was the result in Provi-

dence of the adoption of our present method? For some months after the adoption of the new rule for release to diphtheria patients there was a decrease in the number of cases reported. The same thing took place after disinfection was given up, and for nearly a week in September last, our hospital was empty, and the city was free from any reported case of the disease. While this is not a demonstration that we are right, it indicates that a little more leniency than is usually exercised is not necessarily fatal.

THE PRESIDENT: Gentlemen, the whole subject is now before you for discussion. Dr. Barnes, cannot we hear something from you in this matter?

DR. H. J. BARNES: Mr. President, only in relation to the relative humidity, which Dr. Hill lays so great stress upon. The question arose in my mind when listening to the paper what he meant by high and low relative humidity. We have an average relative humidity in our climate of about 70 per cent. Whether that he considers high or low I don't know. It fluctuates from saturation, 100 relative humidity, down to as low as 35, the lowest that I ever knew recorded in Boston. Once it was recorded as low as 12 per cent in Philadelphia. The mean relative humidity of Denver is 50 per cent, and it goes down to 10 per cent, and up to 100 per cent. So that in listening to the paper some doubt was left in my mind as to what he considers high and low relative humidity. To me a high relative humidity would mean 80 or 85 per cent moisture or above that, and a low relative humidity below 50 per cent.

If you were to fumigate in an atmosphere naturally constituted as it is in the summer time, without material elevation of inside temperature, you might have a relative humidity moist enough for the purposes of which he speaks. But in the winter time, owing to artificial heat, we depress the relative humidity very greatly inside a house. I made ob-

servations some years ago in Boston, and found that the mean relative humidity of our houses was about 30 per cent in winter and it went in one of the hospital wards as low as 12 per cent. That would mean an extremely dry atmosphere. Professor Ward of Cambridge subsequently made observations confirming mine, and Dr. Cowles, who made previous observations in the City Hospital, also reached the conclusion that the mean relative humidity in the winter time in our houses is about 30 per cent, that is, only 10 per cent higher than the kiln which is ordinarily employed for drying lumber, and 12 per cent which was once observed in one of our hospital wards by me was 8 per cent lower than the kiln. The application of this is in the fact that in the summer time we have a naturally constituted air as regards moisture, when it might not be necessary to increase the moisture in applying the formaldehyde, but in the winter time we should enormously increase the amount of moisture if we were to make the disinfection effective. The 10 ounces of water which Dr. Hill recommends, without stating what per cent the relative humidity should be, would lead us to very erroneous conclusions, for in the summer time this might be ample, and in the winter it would affect insufficiently the relative humidity of a thousand cubic feet of air.

How are we to know what the relative humidity is? Of course the only accurate way of measuring it is with the wet and dry bulb thermometers, but that involves a knowledge of the vapor pressure at the temperature when saturated and the vapor pressure at the dew point, and then a mathematical problem is involved. This is worked out in the tables which usually accompany the wet and dry bulb instrument used for measuring relative humidity.

I want to call your attention to a little German instrument called the hydrometer, which measures relative humidity in a more simple way and is substantially correct.. It may be two or three per cent out, but it is a very satisfactory instrument to use for the purpose of roughly determining what the

relative humidity is at the time. That instrument consists of simply a dial, with figures running from zero to 100, indicating per cent, and on that dial is attached what is said to be the skin of an egg, susceptible of very great contraction and expansion due to dry or moist air. This acts on the pointer, moving it up or down on the dial, and you read at a glance within two or three per cent what the relative humidity is. So that with this instrument you would be able to determine whether you were getting moisture enough into the room to raise it to 85 per cent, or above, or whether you were having insufficient moisture for the purposes of fumigation.

In the winter when outside temperature is lower than in the house, condensation, in the form of frost or vapor on window-panes, always indicates a saturated atmosphere or 100 per cent.

DR. SWARTS: Mr. Chairman, I think these three papers bring up the same old contention that comes up before every association of health officers, the practical and the theoretical. It is the laboratory on the one side and the work of the health officer on the other. As a working bacteriologist and as an acting health officer I find myself keeping busy whitewashing both sides of the fence. Working in a laboratory I find the desirability of having three negative cultures from nose and throat, as the most desirable, the most practical and the most essential thing that could be called for, in fact, the only thing which should be satisfactory to any health officer. But upon the other hand, when requested to advise local health officers as to what they shall do in certain cases, I find myself on the other side of the fence, questioning the desirability of so much requirement. In other words, what is practical and what is safe should be the work and the aim of the working health officer.

From what we have heard from these papers, especially the first one, a most able paper, we are led to a state of confusion as to the necessity of obtaining the condition of things

in these different ways. We must have certain apparatus and certain delicate instruments to know the condition of humidity. If any one of those six conditions mentioned is lacking we fail in the work that we are endeavoring to obtain. I think as working health officers what we ask for and what we desire to know is, if we take the chances which have been suggested by the last paper and no dire results do accrue, are we not satisfying ourselves, satisfying the demands of health requirements and health rules, and, what is more to the point, perhaps, in our position, satisfying the clients whom we serve, the public? For to work for the laboratory on the one side, and maintain its ideals, and to satisfy the public on the other hand, is an impossibility. If we follow out the requirements of the three negative cultures, or even a single negative culture, we find ourselves led to retaining cases in quarantine as long as three or four months, perhaps, which is first to our minds, possibly, absurd, to the public ridiculous, and brings all our actions into a state of disrepute with the public. But if we can tell them that ten days after the disappearance of the membrane in diphtheria, or in a certain period which has been shown to be practically safe as the result of the majority of the cultures taken, where negative cultures have been shown to occur in say 10 or 14 days in the majority of cases, and if in the practical results we find that the disease does not spread, I think we are safe in assuming that with less trouble and a shorter time of quarantine we are attaining the same results that we will get perhaps if we follow out the laboratory methods.

I am not decrying the practical application of the laboratory work, nor that it should be abandoned; in fact, we are keeping it up with our full vigor in Providence, in Rhode Island, and will continue to do so. But it seems to me we must have a little horse sense, a little practical application of what is fact and not what is theoretical.

PROF. WINSLOW: Mr. Chairman, I have listened with great pleasure to these papers, particularly perhaps to Dr. Hill's, because the clear and logical statement of the case and the incisive and epigrammatic sentences recall so pleasantly the personality of the writer, whom we so much miss here. It seems to me that the whole bearing of the work of the last ten years in this matter of disinfection and isolation is to change our ideas of the point of attack, to concentrate our attention on the place where the most good can be done. If, to use an old illustration, we compare the body to a walled city of medieval times, in which there are certain dangerous individuals, we will say marauders of some sort, it is obviously easier to stop those marauders at the gates of the city to hold them up there, than it is to catch them after they have got out in the surrounding forests. As Dr. Hill has so well pointed out room disinfection is a sort of rough shot gun method of getting the germs after they are distributed in the environment, a method of killing the marauders scattered through the open woods. But we are coming more and more to realize that the greatest danger comes from the fresh germs just as they leave the body, a large majority of infections being due to pretty direct transfer. Although gaseous disinfection is useful to kill the germs that have escaped, the greatest economy of effort comes in concentrating attention on those germs as they leave the body, on the portals from which the germs escape, the discharges from the mouth and the discharges from the intestines.

Dr. Hill said very little about typhoid fever, but that is one case in point. In this country there is a very grave neglect of proper disinfection of the excreta in typhoid fever. We have a death rate in this state of from 15 to 20 per 100,000 in communities where the water supply is entirely irreproachable, and where from a study of the conditions it appears evident that milk causes no large proportion of the disease. This death rate is mainly due to infection carried in a roundabout way by undisinfected excreta, material which gets on

fingers, or is carried by flies, or food, in some way reaching the mouth. In Germany they have only half that death rate, and I believe this is due to the greater care that is exercised in the disinfection of the excreta.

So in tuberculosis. It is well understood now that the maximum effect can be produced by concentrating attention on the sputum of the patient, and it seems to me perhaps that a solution of this matter of isolation can be reached along somewhat the same line. If a tuberculous patient cares for his sputum he need not be strictly isolated. In that disease of course the strict isolation of the afflicted persons is impossible, but by making a patient care for his sputum we secure a practical isolation.

I wonder if it is not possible to a certain extent to attain that in the case of diphtheria, if it is not possible by printed instructions, and perhaps by personal instructions given by the agent of the Board of Health to the infected person and to the members of the family of the infected person, to secure what is as good as isolation, the care of the points from which infection goes. If those persons who are released, according to the rules adopted in Providence, while they still carry with them virulent diphtheria bacilli, can be instructed as to the possible danger, that can be avoided by the use of clean individual tumblers and spoons and utensils, and by the sterilization of personal body clothing, it seems to me that a large gain may be made .

MR. COFFEY: Mr. Chairman. I would like to ask Dr. Chapin what he does, during the progress of the disease, with the other members of a family in which a case of diphtheria develops. Do you quarantine them, or do you allow them to pursue their usual avocations?

DR. CHAPIN: Mr. President, the majority of people are allowed to pursue their usual avocations. A school teacher, or a nurse, or a conductor on the electric cars, or a

letter-carrier, or a milkman, would not be allowed to do so without repeated cultures from throat and nose. And, as I said, the children in the family are never allowed to go to school under four week, unless all of the children have shown two negative cultures from throat and nose.

MR. COFFEY: Mr. Chairman, I confess that just at present I am somewhat puzzled on the question of diphtheria. At Worcester just now we are suffering from an outbreak of diphtheria larger than anything we have had for a great many years. We are pursuing exactly the same course now that we have always pursued and which heretofore has seemed to work effectively. Worcester is a city of 135,000, and we have had, as perhaps some of you know, a very small amount of diphtheria for years. It has averaged something like 225 cases a year for ten years, and that included the year 1900, in which something like 500 cases developed, so that the average number of cases, eliminating 1900, is something a little over 100 a year for a number of years. And yet this year, in August, when the schools were not in session, diphtheria developed, and of a particularly infectious type. It has been so infectious that it has led me to believe that there is something in this that we don't know much about, in other words, there is a factor of susceptibility and immunity to be taken into consideration.

Why is it that heretofore, until this year, in the ordinary family,—I have seen it year in and year out,—one child would have diphtheria and the other children of the family would be about it in the three or four rooms, and not another child in the house would take it, while this year, almost without exception, wherever the disease has developed nearly every member of the family has contracted it? We have taken out of some houses five children in various stages of the disease. It is not at all uncommon this year for three, four, five, six and seven in a family to contract the disease. It started in, as I say, in August, and was not recognized. A

great many of the cases when first brought to our attention were in the septic stage. In some cases the physicians were at fault. They were called and regarded it as tonsillitis, and did not take any cultures, and were not alarmed until the septic stage was reached. Several deaths were due to those two causes, ignorance on the part of the patient in not calling a physician in time, and ignorance, I am sorry to say, or negligence, on the part of some physicians, to take the usual precautions to see whether the disease was diphtheria or not. I am led now to the belief that the factor of susceptibility and immunity plays a large part in the spread of that particular disease, and if in the spread of that disease why not in the spread of others?

We are pursuing exactly the same course, as I say, this year that we have always pursued with splendid results heretofore. We require a single negative culture for release when the patients stay at home; we require two successive negative cultures for release when they are at the hospital. Now, I know, and every practical health officer who is brought in contact with the people knows, that the average isolation in the average family is a farce. It is impossible with the methods and means at our disposal to place at the door of every house an officer to see that the quarantine rules are carried out effectively. We leave at every house, as one gentleman here suggested, a printed circular, giving simple rules for the care of the patient, the clothing, bedding and the excreta of the patient. I am now finishing my twenty-third year as health officer of Worcester, and my experience has been that people are very much afraid of diphtheria when some one else has it but when the average person has contagious disease in his family he does not seem to care if the rest of the town gets it or not.

We require, as I say, one negative culture, and we also require that a release culture shall not be accepted until 10 days have elapsed from the day the culture for diagnosis was taken. I have a theory in regard to that, which I will give

for what it may be worth. If in ten days no other member of the family contracts the disease, isn't it reasonable to suppose that the family is practically immune, that if they are going to take it they will do so within those ten days, considering, always remember, the average amount of quarantine and of isolation, or lack of isolation, which there is in the average household. So that it seems to me if they go ten days without any other member of the family contracting it, it is due largely to the fact that the other members of the family, at that particular time, are to a greater or less extent immune to the disease and will not take it any way. Under those conditions one negative culture is as good as two.

For a number of years I have noticed that the average number of cases in a family would be one, very seldom two, while this year the disease seems to be particularly infectious. At present it is not virulent. Whether this is due to the fact that we are getting the cases earlier now than we were in the beginning of the outbreak, or whether it is due to the fact that the type is less virulent than at first, I don't know. The fact is we have not had a death for about three weeks, while we have had something like 100 or more cases reported in that time. I am led to the belief that immunity and susceptibility are large factors in this whole question. The bacteriologists and the scientists are trying to solve the problem of why it is that some people are immune to certain diseases at times. I believe it is understood that an individual may be immune at one time and susceptible at another. I believe also that the New York Board of Health are now at work on that very problem. They are taking specimens of blood from the ears of patients and testing it to discover the amount of susceptibility or immunity there may be at that particular time in the blood. When that matter is solved, I think we will have a factor which will aid us largely in the controlling of contagious diseases.

Certainly my own experience is, as I have said before, that the ordinary quarantine and isolation as practised in the ordi-

nary household is not very great. In Boston they have a hospital large enough to take almost every case where the house conditions are such that there is no chance of isolation. That we cannot do at Worcester, because our hospital is not large enough, and we never have attempted to do it on that account. Of course the health officers have by law the authority to take them, in spite of their families. But we have not room, and I think that is true of every other city in the state except Boston. Now, we find that a great many people will not allow their children to go to a hospital, but will keep them at home, and I know, and so must every practical health officer know, that it means very little isolation and very little in the way of a quarantine where they are allowed to remain at home.

We do this in addition. If any member of the family is employed in a store, where he is dealing with the public, we make him either send that child to the hospital or leave the house. If he is a letter carrier, or a motorman, or a conductor, or in fact carrying on any work that brings him in contact with large numbers of the public, he must either get out of the house or allow the child to go to the hospital. Outside of that, quarantine in the house, I am sorry to say, is not very effectual.

DR. FRENCH: Mr. Chairman, there has been something said here about immunity of the different members of the family. I would like to ask what the custom is as to attempting to immunize other members of the family, except the patient. I suppose it is not necessary to ask if antitoxine is used on the patient. I suppose that is the general treatment. But is it used on other members of the family? Is an attempt to immunize the other members of the family carried out? Dr. Chapin or some one else can tell from observation what the custom is.

DR. COFFEY: I think at Worcester some doctors are very particular about those things, and will immunize the

rest of the family, while others are careless and won't. We are ready to furnish antitoxine to them. The State Board furnishes an ample supply, and we are ready to give it, and we urge it, but they don't always do it. There is a prejudice among some doctors against using antitoxine very freely, because of the after rash that occurs in some instances, which is very annoying when it does occur, and they are a little in dread of it. It is not done very freely.

DR. ROSE: Mr. Chairman, I would ask if the antitoxine destroys the virility of the bacillus after it is introduced into the individual. Will it destroy the virility of all the rest of the diphtheretic bacilli in the individual?

MR. RICKARDS: If I may be permitted a word or two more, I might answer that first question by saying I know the State Board of Health uses the antitoxine which is returned to it as of no value, in other words, antitoxine which is clouded, for making serum for their diphtheria outfits. There seems to be no specific action between the antitoxine itself and the organism, since that serum will produce a most abundant growth of diphtheria bacilli.

It seems to me that an analogy might be drawn between the question of isolating and releasing diphtheria patients and our police system. In the latter case we have a state prison, which is filled with prisoners who have been caught redhanded, so to speak. These men have committed crimes, it has been proved against them, and they are in isolation. They can be compared with those cases of diphtheria where the full evidence is upon them; in other words, where you have the full clinical signs. Then, again, there are people at large who have committed crimes in the past, perhaps have served their sentences and are again at liberty. Now, those people are a danger to the community in a way. They may not have reformed and may commit other crimes. You may watch them, but you cannot imprison them again until

they have again committed crime; that is, proved "virulent" and a source of danger to others. There you have practically the infected well person. He is not sick himself, and until you prove that he is going to infect somebody else you can hardly isolate him. Then we have, beside that, those people who have been in bad company and are very likely to become criminals. We might compare those to exposed well persons. We don't know what is going to become of them. They may turn out criminals, and they may not. They need prophylactic treatment.

The virulence of diphtheria organisms isolated from exposed well persons is much lower than is commonly supposed. Figures from various sources would seem to place it as low as fifteen to twenty per cent. In the work done by this Association on diphtheria in well persons, while there were only a comparatively small number of virulent tests made, the work then done went to show that the organisms were usually non-virulent. There were a total of about 75 tests made, if I remember rightly, some of them in persons who were exposed and some in those who were not exposed, and it was only a matter, I think, of somewhere round a dozen cases that proved virulent, those that were not exposed to diphtheria, but who carried the bacilli, were almost invariably found with organisms which were non-virulent.

Just one word more, in regard to what Dr. Barnes had to say about humidity as a factor in disinfection. The point seems to be that to get the best results you should have saturation, and the nearer you approach to saturation the nearer you approach to perfection in disinfecting. You can take an immense amount of dry gas, and with an extremely low humidity, such as we get in the winter time in furnace or steam heated houses, it will take a very large amount of gas to disinfect a room. On the other hand, if you have that room practically saturated, however you may accomplish that, whether it be by some machine that will throw steam into the room, or whether you sprinkle the floor, or what-

ever you do, if you can approach closely to the point of saturation, then a very small amount of gas will accomplish the same amount of work as a very large amount of dry gas. That, I think, has been proved conclusively.

One thing more, a point that is not brought out in Dr. Hill's paper, and that is in regard to the use of machines generating formaldehyde directly from methyl alcohol. In some experiments which I carried on some time ago I found that in the majority of cases where methyl alcohol was used, heated and passed over say a glowing copper spiral, or a platinum sponge, or platinized asbestos, even under the best conditions there was a large amount of unchanged methyl alchohol thrown into the room, beside which there was also a comparatively large amount of formaldehyde which was destroyed, or rather it was broken down to water and carbonic acid gas, which gas, so far as we know, has very little effect in destroying disease organisms.

DR. HILL (discussion added in writing).

In reply to Dr. Barnes' question I may say that it is the relative humidity to which I refer in my paper. I am quite aware that the natural relative humidity of different parts of the country varies immensely, from place to place as well as at each place from day to day. Being for some years on the Disinfection Committee of the American Public Health Association, I have endeavored, so far unsuccessfully, to secure a set of statistics on the relative humidities of the chief cities in each state of the Union of such a character as to be of service in these calculations. In prescribing certain mixtures of formalin and water, Dr. Barnes states that the amount of water prescribed would not at times suffice to raise the humidity to the desired point, (which I may say is "the higher the better" preferably 100 per cent (saturation)). But it must be remembered that the formalin itself contributes 60 per cent of its own weight of water to the mixture.

Thus 20 ounces of formalin contains 12 ounces of water;

a mixture of 10 ounces of formalin with 10 ounces of water contains altogether 16 ounces of water while a mixture of 5 ounces of formalin and 15 ounces of water contains all told 18 ounces of water. In other words, this series of mixtures is such that the humidity secured rises in amount as the formaldehyde gas diminishes—and these are exactly the conditions desired—the less gas, the greater the required humidity—the more gas, the less the required humidity—within limits, however for a really dry gas, acting on really dry organisms, is inert. As to the inefficiency of these amounts of water vapor in affecting the humidity of 1000 cubic feet of space, I may say that at ordinary warm temperatures, say from 60° to 70° F. from 17 to 20 ounces of water per 1000 cubic feet is roughly the maximum that the atmosphere can contain. If the atmosphere has a natural humidity of say 33 per cent (which is surely not a high figure for a room recently occupied by a patient) two-thirds only of this 17 to 20 ounces would require artificial addition to secure complete saturation. If the natural humidity be 50 per cent, only one-half this amount need be artificially added. Hence the amounts of water vapor prescribed, quickly and completely evaporated, are usually sufficient to secure the high humidity desired. In the extreme cases described by Dr. Barnes, more water vapor should be generated, but these extremes are not usualy encountered in practice. Under conditions where hygrometric measurements can be made, and the atmosphere actually brought to 100 per cent saturation, a single ounce of formalin instead of the minimum of 5 ounces above prescribed per 1000 cubic feet is sufficient to kill fresh diphtheria bacilli in forty minutes.

Mr. Rickards has pointed out my omission of the method of securing formaldehyde by direct generation from methyl alcohol. He has also pointed out the difficulties and objections easily raised against this method and it remains for me only to acknowledge the oversight.

Mr. Winslow comments on my omission of directions re-

garding disinfection for typhoid fever, but a perusal of my paper will show that I specifically set aside, for present purposes, what may be termed concurrent disinfection, i. e., that to be carried out during the course of the disease—in order to devote myself to what Chapin has so aptly termed terminal disinfection. Moreover, while occupying a large share of your time, I have not been able to more than skim over the subject—and I must crave the indulgence of all those who detect the frequent omissions of matter which might well have been added.

DR. DENNY: It is very profitable to discuss again at this time the release of diphtheria patients, after we have had experience with the different methods. I think when we discussed it before it was too new for us to have any very definite ideas. The only objection to the culture system is that there are some few cases which are detained too long. If we treat these prolonged cases in a different way it is possible to obviate a great many of the objections of that system. Let every case that persists over four weeks be released "on probation" (to carry out Mr. Rickards's simile of the police system). Allow such an individual to go out in the air, and if it is an adult, let him resume his occupation if he can do it without exposing others; but do not let a child go to school and do not let any one resume work who has an occupation which would endanger others. I think if we can specialize on those few cases that persist over four weeks we can do away with a great many of the objections to the cultural method. In an intelligent community, where the culture method has been thoroughly carried out and the people understand about it, I think it would be very difficult to take up a time limit. People would be very much afraid to have their children play with other children, whom they knew might have the germs in their throats. I think it would be very difficult for us in Brookline, for instance, to take up at the present time any other method than the cultural system of release.

DR. HOLDEN: There is one point that in my judgment has not been sufficiently brought out in this discussion, and that is this: The investigation of the unrecognized case. In very many instances of cases reported and recorded, the patient is isolated, and that is the end of infection from that case. The point I wish to make is that the source of infection of that case should be sought for, and sought for with the utmost rigor. In many cases it would be found that there had been an unrecognized case in the family or in individuals with whom the patient had close contact. To illustrate, some four weeks since a case of diphtheria was reported in Melrose. There had not been another case in that vicinity for some time. I went to the house and found that the case was a child about six years old, who had not, so far as I could learn, come in contact with any other suspicious person, except possibly his brother, who had recently come from a neighboring city. Further investigation disclosed that this boy at the time of his coming to Melrose had a very nasty nasal discharge. A culture proved positive and the case was promptly isolated together with .the other one. Now, if the source of infection had not been sought for and found, we might have had half a dozen cases during the time that this patient remained capable of conveying the disease, and as we know, it is the nasal case, often the mild nasal case, that is the most dangerous. I might multiply instances and I recall many, but it is not necessary. And the same rule will also apply to scarlet fever. It is the unrecognized case that is our great danger. We lay great emphasis upon the isolation of the case after we find it, the quarantine of the family, the disinfection of the rooms, but we pay too little attention to looking for the unrecognized case.

MR. J. ALBERT NYHEN: Mr. President, seeing the discussion is about over and we are about to adjourn, I wish to introduce something for the members to think about in the interval existing between now and our next meeting, that

is, the subject of finger print identification as necessary for public record in the vital statistics. That would be a very interesting subject, and it is one that is bound to come up, one that has got to be considered in the immediate future. It would be very interesting to hear that subject discussed at a meeting in the future.

THE PRESIDENT: Is there anything else to be said upon this most interesting subject? If not, Dr. Durgin moves that this meeting now adjourn.

(The motion was adopted, and the meeting adjourned).

STATEMENT CONCERNING JOURNAL.

By Dr. H. W. Hill, Associate Editor.

NOTE.—This communication, relating to the financial condition of the Journal, was received from Dr. Hill too late for presentation at the October meeting of the Association. It is therefore inserted here.—Managing Editor.

In July, 1905, the financial prospects of the Journal, calculated from the accounts as they stood at that time, showed a possible excess of returns over expenditures per year. Unfortunately bad debts—i. e., delinquent subscribers—have developed, who have been repeatedly billed without effect. Present prospects point to the Journal continuing to pay for itself under present arrangements, and while expansion and advance cannot but be slow, if returns are not in excess of expenditures a decided step is gained in merely maintaining a Journal of this character. Hygienists everywhere should support it, and it is now in a position where every additional subscription means the opportunity for further development. The services of the whole staff are given free and ungrudgingly, for the good of the cause, and if hygienists can be induced to see that they should support a journal of scientific and strictly professional aspirations, open to all professional

hygienists in this country, there can be no doubt of its ultimate success. The names of the contributors and collaboration guarantee that only the best of material will be presented to its readers. A renewed and vigorous effort to bring the Journal pointedly to the attention of all hygienists will be made by the new Managing Editor.

MUNICIPAL SANITATION.

By Charles V. Chapin, M. D.,

Superintendent of Health, Providence, R. I.

ISOLATION IN THE MINOR INFECTIOUS DISEASES. The following rules have recently been adopted by the London County Council.

(vi.) Children suffering from the following diseases must be excluded from school for the undermentioned periods: Mumps, for one month; chicken-pox, for at least two weeks, or until every scab has fallen off the scalp or body; whooping cough, for as long as the cough continues, and not less than five weks from the commencing of the whooping.

(vii.) Children coming from houses in which either mumps, chicken-pox, or whooping cough exists must be dealt with as follows: (a) Children in schools other than infants' schools who have not had the disease and all children in infants' schools must be excluded. (b) Children in schools other than infants' schools who have had the disease need not be excluded.

(viii.) Children living in infected houses and excluded from school under section vii. (a) and (b) must absent themselves for the undermentioned periods: Mumps, for such time as the medical attendant dealing with the case deems necessary or, in cases where there is no medical attendant, for three weeks; chicken-pox, for two weeks; whooping cough, for two weeks.

RETURN CASES OF DIPHTHERIA. The London Health Authorities are still investigating the problem of the recurrence of infectious disease in the family after the return of the patient from the hospital. Previous reports have been made by Drs. Simpson and Cameron. The last report is by Dr. F. M. Turner*. In Dr. Cameron's series the number of return cases was 1.2 per cent of discharges. In Dr. Turner's series it was 1.1 per cent. In Dr. Cameron's group cultures were rarely taken before the discharge of the patient, but the custom has since been inaugurated in the London hospitals of requiring negative cultures before discharge, though there is no general rule as to the number of cultures required. The slight decrease in the proportion of infected cases leaving the hospitals since cultures have been employed cannot be attributed to this practice, for Dr. Turner states that in the North Eastern Hospital, where three throat and two nose negatives are required, the infectivity rate has actually risen.

SANITARY ENGINEERING NOTES.

By Robert Spurr Weston, Assoc. M. Am. Soc. C. E.

EFFECT OF COPPER ON WATER BACTERIA.**

The authors' experiments show that harmless bacteria in water supplies are not appreciably affected by certain small doses of copper sulphate sufficient to kill the harmful kinds of bacteria. They further state that carbon dioxide lessens the destroying power of copper sulphate, but that it has no effect upon the bactericidal action of metallic copper.

The authors fail to state what the effect would be upon the other plant and animal life to which bactericidal doses of copper were added. Others experience goes to show that

*Abstract of report in "Public Health," October, 1906, p. 30.

**Kellerman and Beckwith, U. S. Bureau of Plant Industry, Bulletin No. 100, Part VII.

fish are killed when copper is added to water in large doses.

The authors mention the use of copper sulphate in connection with water purification and advise that in mechanical filtration of hard waters copper sulphate be added some hours before coagulation, since after the addition of sulphate of alumina a hydrate of copper and aluminum appears which precipitates as under direct bacteriacidal action.

The authors state that where copper sulphate and iron sulphate are used together in the purification of water, the copper retains its toxic properties. The latter diminish, however, in the presence of carbon dioxide. When copper salts are used in connection with slow filtration plants they should be added to the effluent, otherwise the toxic effect is lessened.

SULPHATE OF IRON AND CAUSTIC LIME AS COAGULANTS IN WATER PURIFICATION.†

The use of sulphate of alumina as a coagulant in water purification is quite generally understood. Also the use of caustic lime as a reagent for water softening. Furthermore, lime and sulphate of iron in combination have been used to precipitate sewage for many years. Of late years this combination of chemicals, used originally for sewage precipitation, has been adapted to water purification. The causes of this development have been the popular prejudice against the use of alum, the low price of sulphate of iron (a by-product of the manufacture of steel) and lime, and the softening action of the lime when added to water containing calcium and magnesium bi-carbonates. The adaptation of this latter process has not been an easy task, because of certain physical and chemical difficulties. The reaction is not so speedy as that with sulphate of alumina, although when the calcium is once precipitated it acts much faster than does aluminum hydrate, which is the result of the addition of alum or sulphate of alumina to water.

†J. W. Ellis,—paper before Central Water Works Association, 1906. Also Engineering Record, 1906, page 439.

To begin with, the iron hydrate, which is the active coagulating agent, is precipitated in the soluble ferrous or unoxidized form and slowly oxidized to the insoluble ferric form. The coagulant must be oxidized or it will not be removed in the purification process. The main retarding agent is carbonic acid. To remove this lime is added, in excess not only of the carbonic acid but of the calcium and magnesium bicarbonates also. This addition also softens a water and, where magnesium is present, throws down magnesium hydrate, which also acts as a coagulant.

The process is particularly adapted to turbid waters containing calcium and magnesium bi-carbonates. Soft waters and those containing dissolved color have not yet been successfully treated by this process,—probably because of lack of scientific experience rather than because the process cannot be applied. The waters of the Southern seaboard are also difficult to treat,—probably because of the small amount of magnesia contained in them. Experience has shown that the precipitate of calcium carbonate formed by the addition of lime to water crystalizes and settles very slowly.

In any case the process must be controlled by a competent chemist and ample means must be employed to effect precipitation of the carbonates, lest "after deposits" be formed in filters and distribution systems. The sedimentation of turbid waters treated by this process is very rapid, because of the relatively high specific gravity of the iron hydrate (ferric hydrate). It eliminates the necessity of preliminary sedimentation often necessary where sulphate of alumina is used. Smaller coagulating basins may be used and sudden fluctuations in turbidity may be treated successfully without excessive amounts of chemicals. Most of the work of purification is therefore done in the coagulating basin, thereby relieving the filters and increasing their net capacity, as they do not require washing so frequently.

The decreased cost of the treatment is a great advantage; especially is this true where the amounts of suspended matter

are large. In some cases the cost may be only one-half of the cost of sulphate of alumina. The process is already in use in several cities, (St.. Louis, Ashland, Ky., and others), and is to be adopted in Columbus, O., Cincinnati, O., and New Orleans, La..

LABORATORY NOTES.

By Frederic P. Gorham,
Associate Professor of Biology, Brown University, and Bacteriologist, Providence Health Department.

The rapidly increasing literature regarding the Protozoa contains many new methods of cultivating and staining these organisms. In the February number of this Journal we noted methods for isolating Trypanosomes and we now give some of the more recent methods for the cultivation of Amebae, the staining of the Spirochetes of syphilis, and the Negri bodies of rabies.

CULTIVATION OF AMEBAE. Musgrave and Clegg* recommend as a routine procedure for the isolation of amebae from water, etc., but not from stools the following method: To 100 to 500 cubic centimeters of the material to be examined add 0.5 to 1.0 cubic centimeter of ordinary 1 per cent alkaline nutrient bouillon to every 100 cubic centimeters of the sample. Set aside for 24 to 72 hours, when an examination usually shows amebae on the surface. Spread a loopful of this culture over the surface of a Petri dish containing the following medium: Agar 2 per cent, sodium chloride 0.3 to 0.5 per cent, beef extract 0.3 to 0.5 per cent, made as ordinary nutrient agar, 1 per cent alkaline to phenolphthalein, with or without the addition of 1 per cent peptone. In 6 to 48 hours amebae on the surface can be seen readily.

For stools the above method is not satisfactory, because of the selectiveness of amebae from the intestine for certain

*Publication of the Biological Laboratory, Bureau of Government Laboratories, Manila, No. 18, October, 1904.

bacteria. For the isolation of amebae from stools the surface of several plates should be smeared with pure cultures of various kinds of bacteria, preferably isolated from similar stools, and then inoculated with the intestinal contents. It will be found that amebae will develop upon one or more of these plates but not upon all.

To get a pure culture of amebae in either case the procedure is as follows: Select a plate culture on which the parasites are well distributed, place on the stage of a microscope and find, by searching with a Zeiss AA objective, a point where the amebae are some distance apart, swing into position a clean and perfectly dry DD lens and gently lower it until the entire surface is in contact with the medium, raise the lens, remove it from the microscope and gently rub it over the surface of a Petri dish containing the medium. Examination of the two plates will determine whether the amebae has been picked up or not. If properly performed this will give a pure culture of the amebae and a culture of bacteria which may or may not be pure. To obtain a pure culture of the accompanying bacteria several concentric rings of the organism with which it is desired to grow the amebae are made on a sterile Petri dish containing the medium, and a small smear inoculation of the mixed culture is placed in the middle of the smallest ring. The amebae as they multiply will spread over the plate, and in from 24 to 48 hours will have passed through one or more of the rings, and in so doing will have lost the organisms with which they started and have taken up those which form the rings. Inoculations from the outer rings will give the desired pure cultures.

STAINING SPIROCHETA PALLIDA. Goldhorn recommends* the following technique for smear preparations of Spirocheta pallida:

After sufficient curettage at edge of lesion to cause the appearance of a small amount of serum and blood, make im-

*Journal of Experimental Medicine, VIII., 1906, 451.

pression preparations. If lesion is in mouth or vagina, after gentle curettage, remove a little blood with a platinum needle and smear on slide. Blood smears from lobe of ear are usually negative. For staining the author remarks that "the Giemsa staining method, which is based upon the Nocht-Romanowsky principle of staining is so time-consuming where the question of a possible quick differential diagnosis is to be considered as to be quite useless .. . . . . Besides this, we have to deal with the disadvantage that the organism is stained so lightly as to be readily overlooked. One can safely state that there is no modification of the Romanowsky method which will not stain the pallida but I believe the results to be less brilliant than those obtainable by the method I am about to describe."

To 200 cubic centimeters of water in which 2 grams of lithium carbonate are dissolved add 2 grams of methylene blue, (Merck's medicinal, or Grubler's BX, or Koch's rectified). Heat moderately in a double boiler until a rich polychrome is formed. This may be determined by examining a sample every few minutes in a test tube against artificial light; a distinctly red color denotes the proper stage. Allow to cool spontaneously, filter through cotton, and to one half the filtrate add gradually 5 per cent acetic acid until litmus paper shows a distinct acid reaction above the line of discoloration, then add the remaining half of the stain. Add gradually, while stirring, an approximately one half per cent solution of French eosin, until a filtered sample shows the filtrate to be of a pale bluish color with slight fluorescence. Allow to stand one day, filter, collecting the precipitate on a double piece of filter paper, dry at room temperature or not over 40 degrees C. When completely dry dissolve in commercial wood alcohol, allow to stand one day in an open vessel and filter.

To stain the smears drop sufficient stain on the unfixed smears to cover and allow to act 3 or 4 seconds, pour off the excess stain and introduces the slide slowly into clean

water, film side down, hold in the water 4 to 5 seconds, then wash off the excess stain. Pallida should appear violet in color and this may be changed to bluish black by flooding the slide for 15 to 20 seconds with Gram's solution. Wash and dry.

DIAGNOSIS OF RABIES. For rapid diagnosis one of the best stains for the demonstration of the Negri bodies is that of Van Gieson. It is employed regularly at the Laboratories of the New York Department of Health, the New York Pasteur Institute, and has been found extremely satisfactory in our own laboratory. The technique is as follows:

1. Cut away the top and occipital region of the skull and without removing the brain take pieces the size of a BB shot from the cervical cortex in the region of the crucial sulcus, from the cortex of the cerebellum and from the hippocampus major.

2. Place one of these pieces half an inch from the left end of a slide which has been cleaned with soap and water, acid alcohol and the free flame, cover with a clean cover-glass, press down gently until the brain substance spreads to the edges of the cover. Draw the cover-glass to the right, slowly and evenly across the slide.

3. The smear thus made is placed with or without drying into wood alcohol for one to three minutes, and dried in the air.

4. Prepare a stain as follows: 2 to 3 drops of a saturated alcoholic solution of rose anilin violet, 4 to 6 drops of a 50 per cent solution of methylene blue, 20 cubic centimeters of distilled water. Flood the slide with the stain, heat gently until steam arises, wash in water, dry with filter paper.

In well made and well stained smears the Negri bodies appear as pink, crimson or magenta inclusions in the blue nerve cells. The nuclei of the nerve cells are blue and the blood corpuscles are yellowish red. Whether the bodies stain pink, crimson or magenta seems to depend upon the condition of the material examined and upon the proper

mixtures of the stains. It will often happen that a mixture which is successful in one case will be worthless in another and the mixture must then be modified by using a little less rose anilin violet.

In addition to the above method that of Giemsa* or that of Mallory‡ though longer and more complicated, is preferred by some. The section method § is still longer and more tedious and has nothing to recommend it over the smear method.

BOOKS REVIEWED.

The second report of the Wellcome Research Laboratories, Gordan Memorial College at Khartoum, Sudan, has been received from the director, Dr. Andrew Balfour. The functions of the Laboratories as outlined by the director, are:

a. To promote technical education.

b. To promote the study, bacteriologically and physiologically, of tropical disorders, especially the infective diseases of both man and beast peculiar to the Sudan, and to render assistance to the officers of health, and to the clinics of the civil and military hospitals.

c. To aid experimental investigations in poisoning cases by the detection and experimental determination of toxic agents, particularly the obscure potent substances employed by the natives.

d. To carry out such chemical and bacteriological tests in connection with water, food stuffs, and health and sanitary matters as may be found desirable.

e. To promote the study of disorders and pests which attack food and textile producing and other economic plant life in the Sudan.

f. To undertake the testing and assaying of agricultural,

*‡Williams and Lowden, Journal of Infectious Diseases, III., 1906, 460.
§Frothingham, Journal of Medical Research, XIV., 1906, 471.

mineral and other substances of practical interest in the industrial development of the Sudan.

The volume was received too late for a detailed report in this number. A somewhat cursory examination reveals very much to interest the sanitarian. The book itself is well bound, well arranged and excellently illustrated.

In addition to an interesting report on the routine work of the Laboratories, a report of the Chemical Laboratory by Dr. William Beam and a report by the Travelling Pathologist and Naturalist, Dr. Sheffield Neove, the book contains special articles on "Mosquito Work in Khartoum and the Anglo-Egyptian Sudan," "Biting and Noxious Insects other than Mosquitos," "A Haemogregarine of Mammels," "A Leucocytozoon of Mammals," and "Changes in the Erythrocytes of the Jerboa." Mr. F. V. Theobald, Consulting Entomologist, furnishes a chapter on Culicidae, Human and Animal Pests and Vegetal Pests. Mr. E. E. Austin of the British Museum contributes an article on "Biting Flies of the Sudan."

It is of interest to note that the founder of the Laboratories, Mr. Wellcome, and the Chief of the Chemical Section, Dr. Beam, are Americans. B. R. R.

THE SANITARY CONFERENCE.

The Sixth Annual Conference of Sanitary Officers of the State of New York was opened at Syracuse by Dr. D. M. Totman, the efficient health officer of the city, on Wednesday afternoon, October 24th, a large number of sanitary officers convening in the City Hall. After addresses of welcome from various prominent local authorities an extensive program extending over several days was presented. Lack of space prevents the presentation of the details of the program here. Some of the subjects treated were the diagnosis, prevention and treatment of communicable diseases; the examination of water; and disinfection and disinfectants.

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